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THE ORIGIN OF LANGUAGE[1]

By Dr. E. L. THORNDIKE

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NOBODY knows when, where or how speech originated, and I am stepping in where wise scholars in linguistics and psychology fear to tread. My colleagues in psychology will, I beg, permit this divagation into speculation by one who has labored long in the less exciting fields of experiment and statistics. I ask and expect no mercy from experts in linguistic science, but only that they build a better theory on the ruins they make of mine.

We must first glance at three time-honored and then dishonored theories, now known by these opprobrious names: ding-dong theory, bow-wow theory and pooh-pooh theory.

The ding-dong theory assumed a mystical power of certain things to evoke certain sounds from men. Since each such sound was associated with the experience of the thing, it came to mean it. And since men were alike in their responses to things by sounds, one of these sounds meant more or less the same thing to all in the group, and easily became a vehicle of communication. All the evidence is against the existence of any such mystical power, and only extremely strong evidence would induce any scientific student of psychology or of language to put any faith in so extremely unlikely an origin of language.

The bow-wow theory supposed that men formed habits of using the sounds made by animals, things or events to mean the respective animals, things and events and that these habits started them on the road to inventing other sounds as signs of animals, things or events. For various reasons this theory is discredited. Doubtless after man has language, he will

[1] A lecture given on November 5, 1942, as one of the series of the William James lectureship at Harvard University.

often make the sounds that animals and things make, but it is doubtful how often he will do so in a languageless group. Possibly he will do so only accidentally as a part of his general vocal play. There might be little agreement in the ideas evoked in the members of a human group by hearing the varying sounds which its various members made when they thought of a dog, a cow, thunder and the like.

Even if a group got a sufficient agreement in the case of forty or fifty sounds for these to be used commonly in the group, an advance by the addition of non-mimetic sounds as signs of things and events would be difficult. If the mimetic sounds remained fully mimetic, it might well be impossible. But the opponents of the bow-wow theory have not considered sufficiently the possibility that a human group might modify their vocabulary of mimetic sounds by slurring, abbreviation and other processes that make speech easier for the speaker without losing the old meanings of animals, things and events in the hearer. If close imitations of a dog's barking, cock's crowing, baby's crying, lamb's bleating, etc., became conventionalized within a human group into sounds no more like the originals than *bow-wow, cockadoodledoo, mama* and *bah-bah,* or *urr-urr, uk a duk a duk duk, na-na* and *buh-buh,* that group could in a few generations progress to a set of sounds many of which would mean primarily certain animals or things and only secondarily or not at all the sound made by the respective animals and things. The group's vocabulary would all be about things that had distinctive sounds, but could be in the form of sounds different from these and in some cases hardly suggestive of them. The invention of a non-mimetic sound for some thing hitherto nameless would then be easier. The use of such an invention would, of course, spread somewhat slowly within the group and very slowly outside it to groups accustomed only to mimetic words.

The pooh-pooh theory, or interjectional theory, supposed that the instinctive unlearned cries of man as a wordless animal, which already are sounds that are evoked by certain situations and evoke in human hearers certain equally unlearned responses of action and feeling, came to possess meanings also, and that on the basis of this vocabulary of familiar sounds meaning pain, surprise, fear, affection and the like, early man here and there used other sounds to mean other facts.

Nobody should doubt that part of this is true. To a mother whose baby cries and seeks her breast that cry probably means that the baby wants to be fed if anything means anything to her. If she can think of anything she will think of that, as well as react appropriately to it. But for various reasons students of language have decided that the attachment of meanings to the hearing or the making of these sounds of instinctive nature is not adequate to originate articulate speech. So-called animal language plus the power of thinking meanings would not produce human language.

An ingenious theory has been set forth by Sir Richard Paget, a physicist and student of phonetics, who argues that the total behavior of a man to a situation includes characteristic movements of the tongue and lips and other organs of speech. These gestures of the mouth parts became specially important when a man's hands were "in continual use . . . for craftsmanship, the chase, and the beginnings of art and literature," so that he could not gesture with them. Sounds were added to these "mouthings," and finally came to play the leading role. In Paget's own words:

> Originally man expressed his ideas by gesture, but as he gesticulated with his hands, his tongue, lips and jaw unconsciously followed suit in a ridiculous fashion, "understudying" (as Sir Henry Hadow aptly suggested to me) the action of the hands. The consequence was that when, owing to pressure of other business, the principal actors (the hands) retired from the stage—as much as principal actors ever do—their understudies—the tongue, lips and jaw—were already proficient in the pantomimic art.
>
> Then the great discovery was made that if while making a gesture with the tongue and lips, air was blown through the oral or nasal cavities, the gesture became audible as a whispered speech sound. If, while pantomiming with tongue, lips and jaw our ancestors sang, roared or grunted —in order to draw attention to what they were doing—a still louder and more remarkable effect was produced, namely, what we call voiced speech. . . .
>
> In this way there was developed a new system of conventional gesture of the organs of articulation from which, as I suggest, nearly all human speech took its origin. . . .
>
> We can now form a mental picture of how the process of speech-making actually began, but an example or two will make the argument clearer. If the mouth, tongue and lips be moved as in eating, this constitutes a gesture sign meaning "eat"; if, while making this sign, we blow air through the vocal cavities, we automatically produce the whispered sounds mnyΔm–mnyΔm (mnyum), or mnIΔ-mnIΔ (mnyuh)—words which probably would be almost universally understood, and which actually occur as a children's word for food in Russian, as well as in English. . . .
>
> Another adult example may be given, namely, in connection with the beckoning gesture—commonly made by extending the hand, palm up, drawing it inwards towards the face and at the same time bending the fingers inwards towards the palm. This gesture may be imitated with the tongue, by protruding, withdrawing, and bending up its tip as it re-enters the mouth and falls to rest.
>
> If this "gesture" be blown or voiced, we get a resultant whispered or phonated word, like eda, eda or edra (according to the degree of contact between tongue and upper lip or palate) suggestive of the Icelandic hadr, the Hindustani idhar and the Slavonic idei—all of which bear much the same meaning as our English word

"hither." If the same tongue gesture be finished more vigorously, the resultant word will end in a *k* or *g*, owing to the back portion of the tongue making a closure against the soft palate.

Thus, by unconsciously using the tongue, lips, jaw, etc., in the place of the head, hands, etc., pantomimic gesture would almost automatically produce human speech.[2]

Paget fabricated words by moving his own jaws, tongue and lips in ways which seemed to him likely to have been used as oral gestures of primitive men accompanying manual or other gestures meaning reach up, draw back suddenly, scrape, wave aloft, shoot with a bow and arrow, sew, blow, plough, strip grains from the stalk, pick berries, collect them and bury them in the ground, and many others. He finds substantial correspondences between his fabricated sounds and certain words in old languages. Of the famous Aryan roots he considers that 77 per cent. are clearly pantomimic. For example, "tank—contract, compress—as in thong, is due to two compressions in succession fore and aft the palate." "da—give—seems to be an offering gesture made with the tongue."[3]

Paget's book "Human Speech" is so recent (1930) that his theory has not yet received a pet name. Using the first illustration that he gives we might call it the yum-yum theory. This, however, really misrepresents and unduly favors it; for the theory requires the mouth parts to pantomime not eating, drinking, sipping, blowing and other acts of the mouth parts themselves (nobody doubts that), but movements of other parts of the body. A truer nickname would be the "tongue-tied" theory, meaning that the tongue is yoked with the body by subtle bonds of mimetic kinship. The theory has been accepted by at least one psychologist, Eisenson, but it has not been acceptable generally. Personally, I do not believe that any human being before Sir Richard Paget ever made any considerable number of gestures with his mouth parts in sympathetic pantomime with gestures of his hands, arms and legs, still less that any considerable number of men in any local community made the same oral gestures in such pantomime.

And now for my theory, which is a humdrum affair compared with any of these four.

Let us assume a group of one or more human families living together at least as continuously as one of the groups of chimpanzees studied by Nissen in their natural habitat. Let us assume that their environment includes, besides the untouched objects of nature, a few objects chosen and preserved as tools, say a few pounders, a few cutters, a few gourds, shells or other dippers and holders, and perhaps a few stabbers and scrapers; and also some natural objects chosen and preserved as playthings, things that one can chew on, roll or throw, make a noise with, and the like.

We may safely assume further that these humans made a wide variety of movements with their hands, much the same as the human infants of to-day instinctively make, pushing, pulling, tearing, putting into their mouths, dropping out therefrom, dropping, throwing, picking up, etc., etc.

We may safely assume further that these humans made a variety of sounds like the meaningless prattle of infants, letting their mouth parts play with their voices in the same multifarious way that their hands play with any obtainable object. The variety of sounds made may indeed have been greater than that made by an infant of to-day, whose vocal play may be narrowed by the elimination of sounds which are alien to the language which his environment favors. And we know that an infant of to-day makes a much wider variety of articulate sounds than the language of his parents contains.

Such a person in such a group would at an early age have a memory image or expectation or idea of the appearance of the person who nursed him, which her voice or smell or caress could evoke though she was unseen. By having been experienced in so many different contexts, some image or expectation or idea referring to her would have acquired an existence independent of any particular sequence of behavior. In a similar manner he would have an image or expectation or idea referring to each object that had been associated with many varying concomitants in his uses of it or play with it.

Such a person would prattle while he worked or played much as a child of a year or two now prattles as he plays. If his making a certain sound became connected with his experiencing a certain object or act and having an image or expectation or idea of that object or act, he would have a language. That sound and the act of making it would mean that object or act to him. It would be a private language useless as yet for communication. It would be a narrow language consisting of only a few words referring mostly to his own acts and possessions, to the persons in the family group and to their acts and possessions. But it would be genuine language.

And it would be a valuable intellectual tool for its possessor, enabling him to replace the somewhat cumbrous and elusive images or expectations by sounds that he could make and arrange more or less at will. If he did connect *ik* with his digging stick, and *üg* with his large turtle-shell container, *yum* with truffle and *kuz* with clam, he could plan an expedition to get truffles or one to get clams more easily and conveniently than he could with only pictorial memories. Consequently, we may safely reckon that any person

[2] R. Paget, "Human Speech," pp. 133–138, *passim*, 1930.

[3] *Ibid.*, p. 149.

who made these connections that gave sounds meanings and gave things symbolic equivalents would keep them, even though he alone understood them.

What now is the probability that a person brought up in a languageless family group would form one such connection whereby a sound (not an instinctive cry of pain, delight, triumph, etc.) meant an object? What is the probability that he would form two such? Three? Four? A dozen? A score?

Properly planned experiments with enough infants brought up in a languageless environment for ten years (perhaps for a much shorter time) would give a decisive answer. I have long wished to make systematic observations of infants in linguistically underprivileged environments, but have never been able to find the time, and must rely upon memories of casual observations of my children and grandchildren in making my estimates.

I think that the probability that a person in the top half of the species for intelligence by birth would make four or five such connections is very high, say seven out of ten. Consider a child of early man playing with a large shell used as a container in the household and prattling as he plays. Let us take the state of affairs least favorable to connecting the sound *üg* with that shell.

Let his prattling possibilities consist of a thousand syllables all equally likely to occur, and all as likely to occur in any one situation as in any other. Then the chance that he will utter *üg* as he puts a pebble in the shell is 1 in 1,000 if he prattles at all. And unless that connection between the manual act and the vocal act is somehow strengthened, he will be as likely the next time that he drops a pebble into that shell to utter any other sound in his repertory as to utter *üg*. Very often he will utter other sounds and no progress will be made toward the attachment of meanings to his utterances.

But there are forces which tend to cause progress away from purely miscellaneous vocal play. First of all the child who puts one pebble in the shell is likely to put another in then and there. His enjoyment of the act makes him repeat it, that is, strengthens its connection with the mental set in which he did it first. Now that mental set happened at that time to evoke also the vocal play of saying *üg*, and the confirming reaction which the enjoyment of the manual play set in action tends to spread or scatter so as to strengthen also the connection of the situation with the utterance.

In the second place, saying *üg* to the shell and pebble may be itself enjoyable and the connection may thereby be strengthened. Consequently, the probability that the child will drop a second pebble is substantial and the probability that he will utter *üg* therewith if he utters anything is far above 1 in 1,000.

Let us assume provisionally that some active-minded Homo Sapiens did thus connect *ma* with the mother who nursed and fondled him, *ba* with the round black thing that rolled and tossed, *unk* with the club with which he knocked down his prey, and similarly for a dozen or more "words," as we may truly call them. If he did this, what would be the probability that some second person in the group would come to understand these words? And if he did come to understand them, what would be the probability that the first person would come to use them with the intent of having the second person understand them, and so attain the condition of possessing speech as a social tool?

If one person in a hitherto languageless group of two or three dozen souls has reached the stage of a private language of a score of words the probability that some other person in the group will come to understand three or four of his words is much more than infinitesimal.

His companions might well hear him say *kuz* as he dug up a clam or opened a clam or ate a clam, a hundred times in a week. Even if they paid no more attention to his speech than to his personal play, vocal or non-vocal, the sound *kuz* would tend to make them think of a clam more often than of any other one object. And under certain conditions they would be attentive to his speech. For example, in a group digging for clams together, if one cried *kuz* whenever he found a clam, the cry would become interesting to others.

If the group had a dozen or so "bow-wow" (that is, mimetic) words that they used as signals, they would be thereby the more disposed to attend each to the other's vocalizations. If a second person of the group had a private language of his own, though unlike that of the first person in every particular, the second person would be thereby the more disposed to attend to the first person's vocalizations. If the group had a system of mutual influence by gestures, even one utterly devoid of any vocal accompaniments, its members would be thereby a little more disposed to attend to the vocal behavior one of another.

So I would set the probability that in a group of thirty souls, one of whom had a private language of twenty words, some one other person would come to understand five of these words in the course of a moderate lifetime of thirty-five years as well above one in ten thousand, and probably above one in a thousand.

If the family group of say thirty souls has an inventor of a private language of say twenty-five words and say ten of the thirty understand say eight of the words, what is the probability that any one of these ten will use any of the eight words that he understands, use it, that is, to mean to himself the thing or act or event in question? This probability is substantial, but it is not 100 out of 100. Some persons

in such a group will hear and understand a word hundreds of times, but in all probability never say it at all, except accidentally as an element in their meaningless chatter. But some will, when they themselves utter this word in their meaningless chatter or for any re[illegible]understand it as if it were spoken by A. And th[illegible]f saying a word and having it mean something [illegible]ill tend to be satisfying rather than annoying. Meaningful prattle is more satisfying than meaningless and will therefore be more frequently repeated.

If A, the original inventor, hears B or C or D say one of the words to which he, A, attaches meaning when he himself says them, what is the probability that he, A, will understand the word spoken by B? It is not 100 out of 100. The connection *kuz*→*clam* may remain confined to *kuz* said by A, because A is stupid or by nature an extreme introvert or what William James called a lonely thinker, or because of the general tendency of connections to operate only in the way in which they are formed. But A has, by hypothesis, an IQ of 100 or better, and if B goes about saying *kuz* repeatedly and as if he meant something, A is likely to notice what B says, and will at least be more likely to attach the thought of clam to the sound *kuz* when made by B than to attach any other one meaning to it. I should conjecture that the probability of A's understanding B would be well over 25 in 100 and under 90 in 100.

It is perhaps time to attach a name to the theory which I am expounding. Let us save everybody trouble by giving it an opprobrious name from the start! Since it relies on the miscellaneous vocal play of man instead of his alleged mimetic or emotional utterances, it could be called the "babble-babble" theory. Since it starts with languages private to single persons, and progresses gradually toward speech in the full speaker-hearer relation (which, indeed, my exposition has not yet reached) it could be called the "onety-twoty" theory. Since it depends on successive selections of chance variations in sound-reality connections, it could be called the "chancey-chance" or "luck-luck" theory. Or we may combine its two main dynamic features and call it the babble-luck theory.

Let us continue with the luck-luck course of the babble-luck theory.

If B understands *kuz* as spoken used by A and A understands *kuz* as spoken by B, what is the probability that A will come to use the word as a means of influencing B? What is the probability that B will come to *use* the word to influence A?

It is not 100 out of 100. A and B might continue for years to get meaning from one another's use of the word, but never use it for any purpose other than as a self-reminder or as an aid in personal plans or for self-entertainment. However, if A said *kuz* when he was about to set forth to dig clams, and B was moved by hearing *kuz* to set forth to dig clams also, and so accompanied A on several occasions, there might fairly easily be built up a habit in A of saying *kuz* when he wanted B's company on a clamming trip. (The formation of this habit would not be as simple as this sounds or by one direct linking, but by various cooperating associative links which I could describe if necessary). Or if A had already a habit of purposive communication with B by means of a gesture such as pointing to a clam and to B's mouth when he wished or permitted B to eat it, A might well happen to say *kuz* along with the two gestures and eventually in place of the former gesture. (Here again the substitution would not be as simple as it sounds, but it could come to pass.)

A and B thus reach a stage where a word is used by one of them, say by A, with the expectation that his saying it in the presence of the other will produce or favor certain behavior in the other, and where A has the habit of saying it to the other as an appropriate thing to do when a certain desire or purpose moves him. This is genuine human language used in the speaker-hearer relation. But the relation is, as yet, unidirectional, from A as speaker to B as hearer.

Speech need not progress further to full two-way, give-and-take speech, but it could, and often would. I will not run the risk of wearying you with the probabilities that the normal operations of repetition and reward would lead men to this final stage. They are high.

Each of the stages that I have described, from that of words used privately to purposive use of speech in the full speaker-hearer relation, was self-sustaining, by adding something to the group's balance of satisfactions, or to its chance of survival, or to both. A one-man language could make that man remember, anticipate and plan better. In so far as others understood A's words, each of them had some profit from A's experience in addition to their own. In so far as they used his words, each had a private language without originating it. When they reached the stage of understanding one another certain experiences of any one were of profit to all. The stage of purposive use of words to modify the behavior of another gave the possibility of increasing costless cooperation and decreasing costly interference of person with person. Even if the words used were few and the occasions of their use limited to a very narrow round of suggestions, commands, invitations and reports, the benefits would still be enough to maintain the linguistic activities.

Nothing in all this so far requires that either A or B thinks of the other as imagining or meaning clams when he says *kuz*. Such imputation of an inner life to another may arise later and regardless of communication, though of course it can not progress far

without communication. How it arises is a fascinating problem, but to discuss it would make far too long an interruption of our present task.

Let us turn rather to some possible criticisms. First it will be said that the speech which I have derived from babble by luck is a pitifully small, crude affair in comparison with the speech of any known group present or past. This criticism is true. Even after a dozen or more words had been used purposively hundreds of times by a third of the family group and understood after a fashion by two thirds of the group, the use and understanding would be nowhere nearly as clean-cut as that of a modern man or child. A person could use words more or less appropriately in certain situations in the sense that the use of the word was much more appropriate on the average than saying nothing, or than saying some other word of those in his active vocabulary. He could understand words in the sense that what he did to the total situation including the word was on the average different from what he would have done if some other word had been there, and better than what he would have done if no word had been there. But when the imperfect appropriateness of a speaker's uses was combined with a hearer's inadequate understandings, a perfect result could not be expected. If the speaker went much beyond the regular routine uses, he would arouse misunderstanding, neglect or perplexity. The group's linguistic activities might be clumsy as well as extremely narrow.

It will be said that the evolution of any language worthy of the name from such crude beginnings is problematic. This criticism also is true, but it is not very damaging. The problems are no harder than the problems of the evolution of mechanical tools from their crude beginnings. The evolution of a vocabulary of two hundred names of acts, objects and events from a vocabulary of twenty is a problem, though a rather easy one.[4] The evolution of a language that can mean qualities and relations as well as objects and events is a further problem. The evolution of a language that can by sounds ask questions, distinguish orders from statements and date events has further problems. Refinements of meaning, as by our adjectives and adverbs, and abbreviations of speech, as by our pronouns, involve further problems.

I have not solved these and other problems. But I think they are all soluble. If the facts which I have related account for how men came to use articulate words with the purpose of influencing other men, to understand such words and to cooperate in the speaker-hearer relation, they can fairly be said to account for the origin of language, but to leave us with many problems of its development.

A third possible criticism is that the babble-luck doctrine should have produced dozens, maybe hundreds of different languages of this beggarly sort. Origin from miscellaneous babble would cause a multiplicity of primeval languages unless one family group got so great a head start that its language spread to all other tribes before they had invented any languages of their own, which is unlikely. I see nothing objectionable in this. It seems to me sure that any continuing group of intelligent human beings would in time get a language from "babble and luck" if they did not get it earlier from neighbors or visitors who already had it. In many cases they would get it so. Inter-group learning would be of the same general nature as the intra-group learning.

A fourth possible criticism is that hundreds of generations seem to be required to get even this beggarly language if the group has no aid from outside. This seems to be really an argument pro rather than con. Surely the notion that primeval men who were wordless got words as quickly as modern men got Mohammedanism or Christianity or steam engines is fantastic. The length of time from selecting and using flints that were sharp to chipping flints to make them sharp, and the length of time from chipping them roughly to chipping and polishing them in the elegant neolithic styles are both reckoned in many thousands of years.

Whatever may be the value of this account of the origin of meaningful speech, one thing is certain. The human animal's miscellaneous play with his vocal apparatus and the articulate sounds he thereby produces and the associations he makes of these with things and events independently of, and especially contrary to, his linguistic environment deserve much more attention from psychology and linguistic science than they have hitherto received.

OBITUARY

CHARLES FREDERICK MARVIN

Dr. Charles Frederick Marvin, former chief of the United States Weather Bureau, died in the early morning of June 5, 1943, at Doctors Hospital, Washington, D. C., of heart failure following a recent operation. He was born at Putnam (now Zanesville), Ohio, October 7, 1858, son of Charles F. and Sarah A. (Speck) Marvin; and was educated at the public

[4] One generation having reached the linguistic status I have described, the second generation can learn from it and spend most of its linguistic activity in adding its inventions to the parental stock. The custom of naming things and acts by sounds may, after a certain number of such sound-meaning connections has been reached, become a conscious deliberate habit. Some early linguist may then devote his spare time to naming every person in the group, every animal that frequents the locality, and every tool or weapon that he uses.

schools of Columbus, Ohio, and the Ohio State University, which awarded him the degree of M.E. in 1883 and of Sc.D. (honorary) in 1932. From 1879 to 1883 he taught mechanical drawing in this university and instructed in its mechanical and physical laboratories. In 1884 he was appointed a junior professor in the Office of the Chief Signal Officer of the U. S. Army, where he remained until the establishment of the Weather Bureau in 1891. After this date and through several administrative changes, he served as "professor of meteorology" until 1913, when, following the recommendation of the National Academy of Sciences, President Wilson appointed him chief of the Weather Bureau, a position he held until his retirement in 1934, after 50 years of service.

Dr. Marvin's services to the Weather Bureau and to meteorology naturally had to do with several branches of this science. His principal contributions were in the designing, construction and standardizing of meteorological instruments of many kinds; and in guiding the policies of the Bureau during the early stages of its transition into an important influence in the present era of aeronautical expansion. The leadership of the Weather Bureau was his responsibility during troublous times; his inherent qualities of judgment and stability played their part in his administration of the Bureau's affairs.

Above all, his interest in improving the exactness of meteorological measurements stood out. For nearly every weather element—wind direction, wind velocity, barometric pressure, evaporation, cloudiness, precipitation (both rain and snow), duration of sunshine, intensity of sunshine, temperature, humidity—he developed one or more measuring and automatically recording devices, either original or modified, and designed to improve meteorological observations and records for a variety of public uses. One of his most important contributions in this connection, and also one of his earliest, was the experimental evaluation of the constants in humidity equations and the construction of humidity tables; it is from these tables that one can read off at once the current humidity of the air as soon as he knows the simultaneous readings of a properly exposed dry thermometer and an adequately ventilated wet thermometer—information valuable on its own account, and vital to the forecasting of the coming weather of the next few to 24 hours.

Of all the meteorological instruments, however, the Robinson cup anemometer, to measure the velocity of the wind, seemed to fascinate him most. He worked on it, effectively, and wrote about it, from early in his career with the Weather Bureau until years after his retirement. Another of the studies and one of particular significance in early systematic investigation of the upper air was the design and construction of kites and kite instruments.

Besides these distinctly meteorological contributions, he found time and energy to devote to other practical and important interests—notably the design and building of seismographs and the reform of the calendar. One of his seismographs was in operation at the Central Office of the Weather Bureau in Washington, D. C., for many years, and when finally dismantled was one of the best mechanically recording seismographs then in use anywhere. His work with calendar reform, devoted and constant though it was, found the inertia of public conservatism in this subject too great to be overcome by logic. Dr. Marvin was a member of a number of scientific societies, among them the American Geophysical Union; the American Meteorological Society (president, 1926); the Washington Philosophical Society (president, 1903); the Washington Academy of Sciences, and the National Advisory Committee for Aeronautics.

In 1928 he was knighted by the King of Norway to the Order of St. Olaf, first order, in recognition of the aid given by the Weather Bureau to Raold Amundsen during his Arctic explorations; and in 1934 was a U. S. delegate to the League of Nations at Geneva.

He was a member of the Cosmos Club of Washington, D. C.

Dr. Marvin was three times married: to Nellie Limeburner, June 27, 1894, died February 27, 1905; 2d, to Mabel Bartholow, November 8, 1911, died February 8, 1932; 3rd, to Sophia A. Beuter, November 12, 1932, died February 3, 1943. He is survived by two daughters, Mrs. E. Parks Norwood, of Tucson, Arizona, and Mrs. Claud Livingston, of Kenwood, Md., and one son, Charles F. Marvin, Jr., of Washington, D. C., all children by his first wife; and by six grandchildren.

W. J. HUMPHREYS

U. S. WEATHER BUREAU,
WASHINGTON, D. C.

RECENT DEATHS

DR. KARL LANDSTEINER, emeritus member of the Rockefeller Institute for Medical Research, died on June 26 at the age of seventy-five years. Dr. Landsteiner was Nobel Laureate for Medicine in 1930.

DR. STANLEY E. COULTER, emeritus dean of the School of Science of Purdue University, died on June 26 at the age of ninety years.

DR. JAMES MCGIFFERT, head of the department of mathematics at the Rensselaer Polytechnic Institute, Troy, N. Y., died on June 18 at the age of seventy-five years. He had been a member of the faculty since 1892.

DR. GILBERT A. YOUNG, since 1912 until his retirement with the title emeritus in 1942 head of the

School of Mechanical Engineering of Purdue University, died on June 27 at the age of seventy-one years.

DR. GEORGE CURTIS MARTIN, from 1909 to 1924 geologist of the Geological Survey, later consulting geologist, died on June 23. He was sixty-seven years old.

Nature records the death on April 24 at the age of seventy-four years of Lionel R. Crawshay, at one time a member of the scientific staff of the Marine Biological Association and for many years research officer of the Sponge Fishery Investigations in the West Indies and British Honduras, and of Professor Kurt Huber, professor of experimental psychology in the University of Munich, recently executed for "traitorous conspiracy."

SCIENTIFIC EVENTS

THE BIRTHDAY HONORS OF THE KING OF ENGLAND

IT is stated in *Nature* that the following names of men of science and others associated with scientific development appear in the Birthday Honors list of the King of England:

Baronet: Sir John Fraser, regius professor of clinical surgery, University of Edinburgh.

K.C.B.: Dr. N. K. Johnson, director of the Meteorological Office.

K.B.E.: Sir T. Franklin Sibly, vice-chancellor of the University of Reading and chairman of the Committee of Vice-Chancellors and Principals.

Knights: Captain J. P. Black, managing director of the Standard Motor Co., Ltd., and chairman of the Joint Aero-engine Committee; D. A. E. Cabot, chief veterinary officer, Ministry of Agriculture; Dr. H. L. Eason, president of the General Medical Council; Dr. C. S. Fox, director of the Geological Survey, India; Dr. H. Spencer Jones, astronomer royal; J. M. Kennedy, deputy chairman of the Electricity Commission; P. M. Kharegat, vice-chairman, Imperial Council of Agricultural Research, India; E. Macfadyen, chairman of the governing body, Imperial College of Tropical Agriculture; Dr. A. D. McNair, vice-chancellor of the University of Liverpool; Professor J. L. Myres, formerly Wykeham professor of ancient history, University of Oxford, for services to learning; Professor G. P. Thomson, professor of physics, Imperial College of Science and Technology.

C.H.: E. W. Hives, for services in the design of aero-engines.

C.B.: J. M. Caie, deputy secretary, Department of Agriculture for Scotland; W. S. Farren, director, Royal Aircraft Establishment, Ministry of Aircraft Production.

C.I.E.: H. Trotter, utilization officer, Forest Research Institute, Dehra Dun.

C.B.E.: R. Chadwick, chief designer and director, A. V. Roe and Co., Ltd.; Dr. H. L. Guy, chairman of the Gun Design Committee, Scientific Advisory Council; Professor J. Jewkes, deputy director-general of statistics and programs, Ministry of Aircraft Production; Professor J. N. Mukherjee, professor of chemistry, University of Calcutta; R. K. Pierson, chief designer, Vickers-Armstrong, Ltd. (Aircraft); Major R. W. Sharpe, chairman, Agricultural Executive Committee, Berwickshire; Lieutenant-Colonel W. W. Zambra, secretary, Imperial Communications Advisory Committee.

THE NATIONAL FOUNDATION FOR INFANTILE PARALYSIS

TWENTY-EIGHT grants, amounting to $354,370, have been made by the National Foundation for Infantile Paralysis to universities, hospitals, laboratories and other organizations in eleven states to continue the fight against the disease. Dr. Basil O'Connor is president of the foundation.

The grants were recommended by the medical advisory committees at the semi-annual meeting and have now been approved by the board of trustees. The funds which make the program possible are raised annually in January through the celebration of President Roosevelt's birthday.

Sixteen grants, amounting to $216,020, were made for virus and after-effects research. Four of these are on long-term projects being conducted at Yale University, the Johns Hopkins University, the University of Michigan and the University of Wisconsin.

Twelve grants, amounting to $138,350, were made for various educational programs including the training of technicians in the Kenny method of treatment. Some of these grants include projects for educational work for physicians and the public. The sum of $2,500 was appropriated for the preparation of a complete bibliography on poliomyelitis. The compilation is being done for the foundation with the aid of the library of the American Medical Association and the John Crerar Library, both in Chicago.

The list by states follows:

CALIFORNIA

Stanford University School of Health (Women)	$ 11,820
American Physiotherapy Association, Stanford University	10,000
Stanford University	6,800

University of California Medical School, San Francisco	4,650	
Total		$ 33,270
CONNECTICUT		
Yale University School of Medicine for the Yale Poliomyelitis Study Unit, New Haven (1st year of a 5-year grant)		37,200
GEORGIA		
Warm Springs Foundation, Warm Springs		43,480
ILLINOIS		
University of Chicago	$ 7,500	
Northwestern University Medical School, Chicago (2 projects)	6,700	
American Medical Association, Chicago	2,500	
Total		16,700
IOWA		
State University of Iowa, Iowa City		1,000
MARYLAND		
The Johns Hopkins University for the Center for the Study of Infantile Paralysis and Related Virus Diseases, Baltimore (2d year of a 5-year grant)		38,320
MASSACHUSETTS		
The Children's Hospital, Boston (2 projects)	$ 11,400	
Harvard Infantile Paralysis Commission, Boston	10,000	
Massachusetts General Hospital, Boston	3,300	
Total		24,700
MICHIGAN		
University of Michigan School of Public Health, Ann Arbor (1st year of a 3-year grant)	$ 40,000	
Michigan Department of Health, Lansing	19,950	
Wayne University College of Medicine	5,000	
Total		64,950
NEW YORK		
National Organization for Public Health Nursing, New York City (2 projects)	$ 31,100	
National League of Nursing Education, New York City	16,500	
University of Rochester School of Medicine and Dentistry, Rochester	14,800	
Teachers College, Columbia University, New York City	1,750	
New York Medical College Flower and Fifth Avenue Hospitals, New York City	500	
Total		64,650
PENNSYLVANIA		
University of Pennsylvania, Philadelphia	$ 10,000	
D. T. Watson School of Physiotherapy, Leetsdale	4,500	
Total		14,500
WISCONSIN		
University of Wisconsin, Madison (3d year of a 5-year grant)		15,600
Grand Total		$354,370

THE INTERNATIONAL COMMISSION ON ZOOLOGICAL NOMENCLATURE

THE International Commission on Zoological Nomenclature is issuing a series of opinions on cases which have been submitted to the commission prior to 1939 and on which a sufficient number of votes have been received from the commissioners.

Opinions of the International Commission on Zoological Nomenclature were formerly published in the Smithsonian Miscellaneous Collections. This new series of opinions, which begins with No. 134 and goes through 144, are published at the Publications Office of the Commission, 41, Queens Gate, London, S.W. 7.

The commission has also commenced the publication of *The Bulletin of Zoological Nomenclature*, the official organ of the Commission on Zoological Nomenclature; Part 1 of Volume 1 was published on May 21, and the second part was published in June. Subscriptions for the *Bulletin* may also be sent to the Publications Office.

JAMES L. PETERS,
Assistant Secretary

THE TERCENTENARY COMMEMORATION OF THE INVENTION OF THE BAROMETER

IT is announced that a tercentenary commemoration of the invention of the barometer will be held at the University of Toronto on October 19.

There will be two meetings, one in the afternoon and the other in the evening. The program follows:

2:00 P.M. (1) "Telescope, Microscope and Barometer as a Point of Departure for the Natural Sciences." By Louis C. Karpinski, University of Michigan.

(2) "The Effects of the Discovery of the Barometer on Contemporary Thought." By G. S. Brett, University of Toronto.

8:00 P.M. (1) "Subsequent History of the Barometer."

By W. E. Knowles Middleton, Meteorological Office, Toronto.

(2) "The Applications of the Barometer in Physics and Chemistry." By John Satterly, University of Toronto.

In proposing that this celebration be held in time of war the participating organizations are expressing their belief that other forces than those of evil will prevail and that the continuity of human endeavor is worthy of emphasis, particularly in these disturbed days.

THE PITTSBURGH MEETING OF THE AMERICAN CHEMICAL SOCIETY

"CATALYSIS of War Chemistry" will be the central theme of hundreds of papers and addresses to be presented before the one hundred and sixth meeting of the American Chemical Society, which will be held in Pittsburgh from September 6 to 10.

Advances in wartime research will be reported at the scientific sessions, of which Dr. Per K. Frolich, director of the Chemical Division, Esso Laboratories of the Standard Oil Development Company, Elizabeth, N. J., is president.

"Manpower" will be the subject of a three-day symposium at which industrial leaders will discuss measures to overcome the shortages of trained scientific workers essential to the war effort. W. L. Elder, of the War Production Board, will preside.

The food needs of the United States, civilian and military, and of the United Nations will be explored in symposia dealing with recent advances in the chemistry of dairy products, vitamins, proteins and boron in agriculture.

Other symposia will be devoted to anti-syphilitic agents, industrial hygiene, research tools of the colloid chemist, research management in small laboratories, unit processes, paint, plastic, reactions of solids, molecular addition compounds and library technique.

Papers to be read at divisional meetings will outline progress in petroleum, gas and fuel, fertilizers, sugar chemistry and technology, cellulose, water, sewage and sanitation and chemical education. The society's divisions of organic chemistry, physical and inorganic chemistry, biological chemistry, analytical and microchemistry, colloid chemistry and industrial and engineering chemistry will also convene.

Francis C. Frary, director of research of the Aluminum Company of America, has been appointed honorary chairman of the convention. Professor J. C. Warner, of the department of chemistry, Carnegie Institute of Technology, is general chairman. William P. Yant, director of development and research, Mine Safety Appliances Company, is general vice-chairman. An attendance of four thousand is expected.

According to the announcement, in attacking the problems of manpower, first-hand knowledge of the experience of the United Nations will be drawn upon.

The record of the American chemical industry in terms of production has been excelled by none and equaled by few.

The tremendous increase in requirements resulting from a huge construction program, the loss of men to the armed forces, the lack of an officially sanctioned training program for replacements, and the use of college and university facilities by the Army and Navy have created a difficult situation.

In the manpower symposium, both current and anticipated shortages will be discussed. Plans to improve existing conditions will be suggested by experts familiar with the employment of women, with successful training programs, draft deferment and other fields concerned with the problem of recruiting personnel for industry.

The symposium on antisyphilitic agents, lasting two days, will be sponsored by the division of medicinal chemistry, of which John H. Speer, of G. D. Searle and Company, Chicago, is chairman. Technological developments in industry will be traced in a unit process symposium at which Professor R. Norris Shreve, of Purdue University, will preside.

F. J. Curtis, of the Monsanto Chemical Company, St. Louis, will be the chairman of the symposium on research management of small laboratories under the auspices of the division of industrial and engineering chemistry. The divisions of biological chemistry and agricultural and food chemistry will hold joint sessions on vitamins.

Divisional meetings will begin on Monday, September 6, at 9 A.M. and conclude on Friday at 2 P.M. Sessions are planned of the council and board of directors, of which Thomas Midgley, Jr., vice-president of Ethyl Corporation, is chairman. A public meeting at which general addresses will be delivered has been arranged for Wednesday, September 8. The headquarters of the convention will be at the Hotel William Penn, where registration of delegates will begin on Sunday afternoon, September 5.

SCIENTIFIC NOTES AND NEWS

THE Lamme Medal, awarded annually for "outstanding achievement in engineering education," was presented on June 19 by the Society for the Promotion of Engineering Education to Professor Thomas Ewing French, head of the department of engineering drawing at the Ohio State University. The presenta-

tion was made by Dr. Henry T. Heald, president of the Illinois Institute of Technology and retiring president of the society, at its golden anniversary dinner.

DR. DAVID RUSSELL LYMAN, of Wallingford, Conn., was presented with the Trudeau Medal for 1943 at the St. Louis meeting on May 5 and 6 of the National Tuberculosis Association.

FRANK H. SHAW, president of the Shaw Insulator Company of Irvington, N. J., received the John Wesley Hyatt Award, a gold medal and $1,000, for distinguished achievement in plastics during 1942 in recognition of his invention and development of transfer moulded plastics. The presentation was made on June 17 at a dinner in the Waldorf-Astoria, New York, by Dr. Per K. Frolich, president of the American Chemical Society and a member of the award committee. The award was established in 1941 by the Hercules Powder Company.

DR. CARL ALFRED MOYER, assistant professor of surgery at the School of Medicine of the University of Michigan, was recently presented with the annual Henry Russel Award in recognition of research on the physiology of breathing and the effect of various drugs on the respiratory processes.

AT the recent commencement of the University of North Carolina, Dr. T. Grier Miller, clinical professor of medicine at the Medical School of the University of Pennsylvania, in charge of the gastro-intestinal division of the University Hospital, was awarded the degree of doctor of laws.

THE University of Arizona has conferred the honorary doctorate of science on E. C. Slipher, of the Lowell Observatory at Tucson.

AT the seventy-third commencement exercises of Wilson College, the degree of doctor of science was conferred on Dr. Gertrude Rand, research associate in ophthalmology of the Knapp Foundation of the College of Physicians and Surgeons, Columbia University.

THE officers, executive committee and members of the Division of Geology and Geography, National Research Council, for the year beginning July 1, are as follows: *Chairman,* William W. Rubey; *vice-chairman,* Otto E. Guthe; *Executive Committee,* William W. Rubey, Otto E. Guthe, Marland Billings, Monroe G. Cheney, G. Arthur Cooper, Richard J. Russell. *Representatives of Societies:* Marland Billings and T. S. Lovering, Geological Society of America; Paul F. Kerr, Mineralogical Society of America; G. Arthur Cooper, Paleontological Society; Otto E. Guthe and Richard J. Russell, Association of American Geographers; Elizabeth T. Platt,[1] American Geographical Society; Charles H. Behre, Jr., Society of Economic Geologists; George W. Morey, American Ceramic Society; Monroe G. Cheney, American Association of Petroleum Geologists; John A. Fleming, American Geophysical Union. *Members at Large:* Ralph H. Brown, William W. Rubey and Joseph T. Singewald, Jr.

DR. ROBERT M. GATES, president of Air Preheater Corporation, New York City, has been nominated for president of the American Society of Mechanical Engineers.

THE Mayo Foundation Chapter of Sigma Xi held its annual meeting on June 10, when new members were initiated. The officers elected for 1943–44 are: *President,* Dr. R. K. Ghormley; *Vice-president,* Dr. H. E. Essex; *Secretary-Treasurer,* Dr. C. F. Code. The retiring president, Dr. A. C. Broders, spoke on "The Relationship of Birds to the Economy of Man."

SIR FARQUHAR BUZZARD, BT., lately regius professor of medicine at the University of Oxford, has been elected to an honorary studentship at Christ Church.

IT is reported in *Nature* that at the anniversary meeting of the Linnean Society, London, held on May 24, A. D. Cotton, keeper of the Herbarium in the Royal Botanic Gardens, Kew, was elected president in succession to Dr. E. S. Russell.

AT the annual general meeting of the Physical Society, London, held on May 18, the following officers were elected for the year 1943–44: *President,* Professor E. N. da C. Andrade; *Vice-Presidents,* Dr. J. H. Brinkworth, Professor C. D. Ellis, Dr. H. T. Flint, Professor N. F. Mott; *Treasurer,* Dr. C. C. Paterson; *Secretaries,* J. H. Awbery (*Papers*), Dr. W. Jevons (*Business*); *Foreign Secretary,* Sir Owen Richardson; *Librarian,* Professor L. C. Martin.

PROFESSOR RAYMOND C. ARCHIBALD became professor of mathematics emeritus at Brown University on June 30. He has been for thirty-five years a member of the department of mathematics, and was at one time editor-in-chief of the *American Mathematical Monthly.* He plans in his retirement to continue to edit "Mathematical Tables and Aids to Computation," published by the National Research Council since last January, as the organ of a committee of the council of which he is chairman. At the request of a committee of colleagues and former colleagues and students he sat recently for a portrait which has been completed and which will be presented to the university in the autumn.

AT Yale University Dr. William U. Gardner has been promoted to be professor of anatomy and chair-

[1] Although Miss Platt died on May 22, her successor on the division has not yet been appointed. Therefore, to show that the American Geographical Society is represented on the division, it seems best to indicate that Miss Platt was the representative for this period.

man of the department of anatomy in the School of Medicine. In both of these appointments he succeeds the late Professor Edgar Allen, who died on February 5, 1943, while on active duty with the U. S. Coast Guard Reserve. Dr. Harry S. N. Greene, associate professor of pathology and surgery, has been appointed professor of pathology. Albert G. Conrad, associate professor of electrical engineering and co-inventor of a new adjustable-speed electric motor, has been appointed chairman of the department of electrical engineering. Cornelius B. Osgood, associate professor of anthropology and curator of anthropology in the Peabody Museum, has been appointed chairman of the department of anthropology.

DR. RAYMOND B. ALLEN, executive dean of the Chicago Colleges of the University of Illinois, has been appointed, from September 1 next, dean of the Medical School. He will succeed Dr. David J. Davis, who is retiring. Dr. Allen will continue in his present position of executive dean of the Chicago departments, which include the colleges of medicine, dentistry and pharmacy and other institutions associated with these units.

DR. GRANVILLE A. BENNETT, associate professor of pathology at the Harvard Medical School, Boston, has been appointed professor of pathology and bacteriology at the School of Medicine of Tulane University of Louisiana.

DR. BURRELL O. RAULSTON, professor and head of the department of medicine and director of clinical teaching since 1930, has been appointed professor of bacteriology and dean of the School of Medicine of the University of Southern California at Los Angeles. He succeeds Dr. Seeley G. Mudd, who will continue as professor of experimental medicine.

PROFESSOR ARTHUR HOLMES, of the University of Durham, has succeeded T. J. Jehu as regius professor of geology and mineralogy at the University of Edinburgh.

DR. EDWARD DAVID HUGHES has succeeded Dr. Kennedy Orton as professor of chemistry at the University of North Wales, Bangor.

PROFESSOR E. J. SALISBURY, F.R.S., Quain professor of botany at University College, London, has been appointed director of the Royal Botanic Gardens, Kew, the appointment to take effect on September 1.

DR. JORGE ANCIZAR-SORDO, director of the National Chemical Laboratory, Bogotá, Colombia, and president of the Colombian Chapter, American Society of Agricultural Sciences, has been appointed a member of the board of directors of the Institute of Tropical Agriculture of the University of Puerto Rico.

DR. JAMES H. ELDER, of the department of psychology of the Louisiana State University, has leave of absence to enable him to serve as psychologist in the office of the chief signal officer of the War Department, Washington, D. C.

DR. E. RAYMOND HALL, associate professor of vertebrate zoology of the University of California at Berkeley, has returned from a visit of two and a half months to Mexico, where he collected specimens and assisted in a survey of natural resources for the Mexican Government. Professor Hall brought back from the southern end of the tableland of Mexico 650 specimens of mammals, which included pocket gophers, skunks, opossums, weasels, mice and rats. The collection is said to be remarkable for the number of varieties, species and even genera which were found so close together.

DR. CARL C. LINDEGREN, research associate, Washington University, gave a university lecture on "Breeding Yeast for the Wartime Diet" at the University of Illinois under the auspices of the Division of Biological Sciences and lectured on June 11 on the same subject before the staff of Dr. Tom D. Spies at the Hillman Hospital in Birmingham.

IT is reported in *Nature* that the Council of the British Institution of Electrical Engineers has made the following awards of premiums: *Institution Premium:* C. J. Beaver and E. L. Davey; *Kelvin Premium:* Dr. F. Brailsford and R. G. Martindale; *John Hopkinson Premium:* R. A. W. Connor; *Non-Section Premiums:* Dr. E. Friedlander (Ayrton Premium), C. T. Melling (Llewellyn B. Atkinson Premium), J. Swaffield (extra premium); *Wireless Section Premiums:* Dr. R. L. Smith-Rose and Miss A. C. Stickland (Duddell Premium), G. Parr and W. Grey Walter (Ambrose Fleming Premium), Professor Willis Jackson (extra premium); *Measurements Section Premiums:* Lieutenant-Colonel K. Edgcumbe (Silvanus Thompson Premium), M. Kaufmann and W. Szwander (Mather Premium), G. H. Barker and A. L. Hancock (extra premium); *Transmission Section Premiums:* A. W. Thompson and J. C. Wood-Mallock (Sebastian de Ferranti Premium), A. A. Pollitt (John Snell Premium), H. Willott Taylor and K. L. May (extra premium); *Installations Section Premiums:* G. B. Alvey and N. Tetlow (Crompton Premium), Forbes Jackson, W. J. H. Wood, G. Smith and E. Jacobi (Swan Premium), J. R. Taylor and C. E. Randall (extra premium); *Paris Exhibition, 1881, Premium:* J. N. Waite.

THE Amateur Astronomers Association, with headquarters at the Hayden Planetarium of the American Museum of Natural History, has established an award

to be known as the Amateur Astronomers Medal to stimulate interest in astronomy and to commemorate the quadricentennial of the death of Copernicus. The medal will be given from time to time to an amateur astronomer or layman who performs an outstanding service to astronomy.

IT is reported in *The New York Times* that Arthur Curtiss James, railroad industrialist, who died on June 4, 1941, left a gross estate of $37,771,613 and a net of $34,771,702. Charitable, religious and educational institutions share $25,317,154, of which the James Foundation gets $23,030,387. The James Foundation, which was created to aid charitable, religious and educational institutions, will receive eight tenths of his residuary estate. In addition to this it also receives art and real estate worth $574,085 and remainder interests in trusts set up in his will valued at $1,202,682. Mr. James requested the foundation to aid institutions which he had helped during his lifetime. Among these are Amherst College, Hampton Normal College and the Tuskegee Normal and Industrial Institute.

ACCORDING to the *Harvard Alumni Bulletin,* gifts to the university for the year to the date of commencement amounted to $5,144,255, a million and a half larger than the total amount received over the preceding year. The gifts for immediate use ($1,078,574) were lower than for any year from 1933–34. But capital gifts of $4,065,681 were larger than for all except three years of the last ten; and of these the unrestricted gifts—the most valued and most needed by the university—amount to $3,320,884, or to more than the total unrestricted gifts for capital to the university in the eight years preceding. At the commencement ceremonies the marshal, David M. Little, presented to the university on behalf of the class of 1918 a check for $100,000.

THE Academia Nacional de Medicina of Buenos Aires has recently established the Hirsch Medical Scholarship with a fund of 500,000 pesos given by Alfredo Hirsch, of Buenos Aires. Selected students will follow medical studies in the United States or Great Britain for two years, beginning in the middle of 1943. For the first ten years the scholarships will be given for studies on cancer, leprosy and infantile paralysis.

THE San Diego Society of Natural History, whose museum in Balboa Park has been taken over by the U. S. Navy for hospital purposes, has been allowed to retain a considerable section of the ground floor, according to Clinton G. Abbott, director. Here the extensive study collections and departmental libraries have been gathered, and the research and publication activities of the staff are being continued. The main library has been moved to San Diego State College for the duration. The exhibits have been stored.

Nature reports that ten Chinese students, the first to visit Britain under a new scheme operated by the British Council, are now on their way to Great Britain. They have been given scholarships by the council, and will study some branch of engineering and will work at the university or college most suited to their special qualifications. Their training is expected to be of special value both because it will equip them for reconstruction work in China, and because their familiarity with British engineering practice and equipment will furnish a link between Great Britain and China.

DISCUSSION

THE CONCEPT OF CELLS HELD BY HOOKE AND GREW

REFERENCES to historical topics in current text-books of biology, botany and zoology can be counted in most cases on the fingers of one hand. In view of the decided lack of any historical approach, it seems rather anomalous that two concepts that are presented from this standpoint are so often either incorrect or distinctly misleading. The first of these—namely, that Schleiden and Schwann were the real originators of the cell theory and enunciated it before any one else—has been disproved in scholarly fashion by Karling.[1]

The second general error is a misunderstanding of the conception of the cell held by the early microscopists, especially by Robert Hooke and Nehemiah Grew. Almost all the biological text-books, including those of botany and zoology, if they refer to the topic at all, either state or imply that Hooke saw merely the *walls* of cork, pith and charcoal. To be sure, he did see the walls of such cells, and with his ineffective microscope, they must have stood out more clearly in such tissues than in living material.

However, Hooke was perfectly aware that cells in living plants had contents. This is stated very definitely in his "Micrographia," published in 1665. "But though I could not with my *Microscope,* nor with my breath, nor any other way I have yet try'd, discover a passage out of one of those cavities into another, yet I cannot thence conclude, that therefore there are none such, by which the *Succus nutritius,* or appropriate juices of Vegetables, may pass through them; for, in several of those Vegetables, whil'st green, I have with my *Microscope,* plainly enough discover'd these Cells or Poles fill'd with juices, and by degrees sweating them out: as I have also observed in green

[1] J. S. Karling, *Amer. Nat.,* 73: 517–537, 1939.

Wood all those *Microscopical* pores which appear in Charcoal perfectly empty of anything but Air."[2] Or again, in speaking *"Of* Petrify'd Wood," he remarked "and with a *Microscope,* I found, that all those *Microscopical* pores, which in sappy or firm and sound Wood are fill'd with the natural or innate juices of those Vegetables, in this they were all empty, like those of *Vegetables* charr'd."[3] Hooke mentioned repeatedly the *"Succus nutritius,* or natural juices of Vegetables"[4] and was definitely aware that cells in living plants had contents.

According to the preface of his well-known book on "The Anatomy of Plants," published in 1682, Nehemiah Grew began his study of the structure of plants in 1664. His work on plants was much more intensive than that of Hooke. No one can read the philosophical discussions and speculations of Grew and not be impressed by his concept of the plant as a whole, and of its functions, even though he was incorrect in many of his deductions. Grew discussed at considerable length the "Infinite Mass of little *Cells* or *Bladders"* of which the parenchyma of the root, for instance, is composed. He mentioned their size variations, their arrangement in rows—"they visibly run in Ranks or trains"—and their contents. "They are the Receptacles of *Liquor;* which is ever Lucid; and I think, always more Thin or Watery. They are, in all *Seed-Roots,* filled herewith; and usually, in those also which are well grown, as of *Borage, Radish,* etc."[5] In other places Grew referred to such *"Bladders"* as "fill'd with Sap"[6] and as *"Cisterns* of *Liquor."*[7]

From these quotations it is evident that Hooke and Grew fully realized that cells in living plants had contents. Of course they had no knowledge of the internal structure and organization of the cell, of its nucleus and other constituent parts, of the protoplast as we know it to-day. They apparently did not appreciate the importance of the cell as a unit in the organism. However, they thought of liquids or juices moving within the plant through the cells, foreshadowing, unconsciously, much more recent work on hormones, vitamins, viruses and the translocation of substances in plants.

Hooke's use of the term cell is often condemned as a "biological misnomer" because the protoplasm is of course the important part rather than the wall. However, in justification it may be pointed out that in plant tissues, at least, the wall is of marked significance; and further, there is nothing either in the etymology of the word or in its use in ordinary parlance that requires that the cell be empty. The Romans used the word *cella* to refer—among other things—to the cell of a honeycomb or to a storeroom for wine, grain, oil, honey, etc. Certainly in such instances the contents were quite important.

In summary, Robert Hooke and his illustrious contemporary Nehemiah Grew knew that cells in living plants had contents; they did not think that they were merely "empty boxes."

EDWIN B. MATZKE

[2] R. Hooke, "Micrographia," p. 116, 1665.
[3] *Ibid.,* p. 107.
[4] *Ibid.,* also p. 114.
[5] N. Grew, "The Anatomy of Plants," Book II, p. 64, 1682.
[6] *Ibid.,* Book I, p. 25.
[7] *Ibid.,* Book III, p. 126.

THE HYDROLYSIS OF d-PEPTIDES

IT is by now well known that peptidases from many different sources can hydrolyze unnatural peptides containing a d-amino acid radicle. Cleavage proceeds more slowly than it does in the hydrolysis of corresponding compounds containing the l-isomer; Berger, Johnson and Baumann,[1] for example, find that peptidases from chick mucosa, yeast autolysate and malt can split d-leucylglycine about one thirtieth as rapidly as they split the racemic mixture. This is in accordance with the theory of steric hindrance, developed by Bergmann and his coworkers[2] to explain the fact that l-leucyl-d-alanine is split more rapidly than glycylglycine but less rapidly than l-leucyl-l-alanine by yeast dipeptidase and erepsin.

Evidence from different sources suggests that the presence of one isomer may interfere with the hydrolysis of the other. Thus Palmer and Levy[3] found that the presence of d-alanylglycine strongly inhibits the hydrolysis of the l-isomer by chick embryo extracts although it is not itself hydrolyzed. On the other hand, Bamann and Schimke[4] find that the dipeptidase of human ovaries will hydrolyze d-leucylglycine but has little action on the d-isomer when dl-leucylglycine is used as substrate. Both Palmer and Levy, and Bamann and Schimke note that the reaction is inhibited by the products of hydrolysis.

It seems probable that the true explanation of these phenomena lies in the effect of the pH change which accompanies dipeptide hydrolysis.[5] In the alkaline range, the dissociation constants of the dipeptides concerned lie between 8.0 and 8.4, while those of the amino acids lie between 9.8 and 10.0. Even with moderate buffering there is a marked alkaline shift during hydrolysis, so that the pH of the digest rapidly passes beyond the optimum and inhibition of enzyme action ensues. In the experiments of Palmer and Levy the presence of the non-hydrolyzable d-isomer presumably

[1] J. Berger, M. J. Johnson and C. A. Baumann, *Jour. Biol. Chem.,* 137: 389, 1941.
[2] M. Bergmann, L. Zervas, J. S. Fruton, F. Schneider and H. Schleich, *Jour. Biol. Chem.,* 109: 325, 1935.
[3] A. H. Palmer and M. Levy, *Jour. Biol. Chem.,* 136: 407, 1940.
[4] E. Bamann and O. Schimke, *Naturwiss.,* 29: 365, 1941.
[5] G. E. Pickford, *Jour. Exp. Zool.,* 92: 143, 1943.

gave better buffering and the assumption is that the pH of the digest never reached the optimum so that an apparent inhibition resulted. These authors state that the pH optimum was at 7.8, the pH at which they worked, but this is not in agreement with the results of Berger and Peters,[6] who found, with three different buffer systems, that the peptidase of the chick embryo had a pH optimum at about 8.5. The latter agrees well with the more alkaline pH optimum of the salamander dipeptidase.

Failures to detect hydrolysis of the d-isomer in racemic mixtures would result from the more rapid hydrolysis of the l-isomer which, when completely split, would carry the pH of the digest beyond the optimum so that continued slow hydrolysis of the d-isomer would be prevented. It is evident that conclusions with regard to the hydrolysis of d-peptides must not be based on the results of hydrolysis of racemic mixtures, a fact that has been overlooked by many authors, including myself.

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U. S. TEXT-BOOKS FOR STUDENTS IN LATIN AMERICAN UNIVERSITIES

PRESENT SITUATION

MOST texts used for advanced study are French and Spanish, despite the expressed desire of many professors to use United States texts. Texts in English from the United States are not used because the texts are too expensive; the 5 to 1 ratio of exchange, with Mexico for example, makes an imported text there cost 25 pesos that costs 5 dollars in the United States. The texts referred to are mostly those used in such small numbers by advanced classes in specialized subjects that a Spanish translation will not pay for itself.

Yesterday I learned of one course in which the students this year at the Universidad Nacional de Mexico effectively objected to purchasing a 25 peso United States text assigned by their professor. He admitted (to them and to me) that they could not afford it. French and Spanish texts cost less in Mexico where potential volume of sales does not justify publication within the country itself of texts in specialized subjects.

Professors in Latin American universities find United States text-books superior for many courses because these books treat of American materials. For example, a botanical text from the United States in illustrating structure, physiology, etc., employs kinds of plants found in America, whereas the European texts and illustrations treat of unfamiliar kinds of plants characteristic instead of Europe and Asia.

[6] G. Berger and T. Peters, *Zeitschr. physiol. Chem.*, 214: 1, 1933.

REASONS FOR MAKING U. S. TEXTS AVAILABLE IN LATIN AMERICA

(1) Several professors in Latin America have expressed a desire for United States texts and the demand and plea is of constant growth.

(2) As already noted, some United States texts are best because they treat of American materials.

(3) Use of United States texts by Latin Americans in process of intellectual training is tremendously effective propaganda in that it brings fuller understanding of English-speaking Americans to the Latin Americans; evidence is provided of United States accomplishment in science, art and literature. This evidence needs to be placed in the record for the benefit of the many Latin Americans who heretofore have known principally of our accomplishment in material matters—typewriters, automobiles and dollars.

MEANS OF ACCOMPLISHMENT

(1) Learn precisely which text-books are desired and where and in what quantities. (2) Make these available at a cost within reach of the students for whom they are intended.

One United States professor in Mexico and Central America, and two or three in South America, by conference with university professors there, and with guidance from Cultural Relations representatives in United States Embassies, should most effectively obtain the requisite information.

The United States professors might well explain the rental system in use on campuses of some United States universities whereby for one course at a cost of only $1.00 a student has the use of a text-book which costs $7.50 new.

To make copies of these texts available at a price within reach of the Latin American students is a most worthy aim. It seems not unfair to place before the people of the United States the opportunity and responsibility for achieving this end.

Possibly no subsidy at all would be required, or at most a smaller one than at first thought would seem necessary, if Latin American students are as much interested in used copies as are United States students, many of whom obtain their copies at a fraction of the list price. Here is how the system operates:

Professor Torrey in 1941 adopted for his advanced course in biology the new text-book by the two authors Peralta and Smith. Each of 150 students in Professor Torrey's class was required to purchase a copy of the text, which cost $7.50. At the end of the course, 50 students retained their copies, but the other hundred students resold theirs to the local book-dealer at $2.00 each. In 1942, the 150 students in Professor Torrey's class rushed to purchase texts. "First come first served" is the rule. One hundred pupils purchased

used copies at $3.75 each and the 50 pupils who arrived later purchased new copies at $7.50 each. In 1943, Professor Torrey will have possibly 50 pupils in all (the other hundred are in military service or war work—I had 8 instead of 80 in an autumn class) and the campus book-dealer has 50 extra copies that he is glad, indeed, to sell for $2.00 each. This is about the price which can be charged in Latin America.

We (the United States) should get busy now so as to have the texts at the places where they are desired at the opening of the new scholastic year in Latin American universities.

E. RAYMOND HALL
UNIVERSITY OF CALIFORNIA, BERKELEY,
DATED AT MEXICO, D. F., APRIL 9, 1943

THE INSTITUTE OF TROPICAL AGRICULTURE AT TURRIALBA

THE news item in the March 26 issue of SCIENCE relative to the foundation of the new Institute of Tropical Agriculture at Turrialba (Costa Rica) was read with considerable interest here, as we feel that the foundation of said institute represents a definite step forward. Turrialba is also an excellent location because it is there that the U. S. Department of Agriculture Rubber Field Station, directed by Dr. Theodore Grant, is located. Thus the newly announced rubber research program of the new agricultural institute will utilize both the experience and the disease-resistant material already developed by Dr. Grant and his staff. This represents a tremendous saving in time, precisely at the moment when time is the most important consideration.

The foundation of the agricultural institute is also of great value because it will contribute to giving both scientists and government officers in the United States a truer picture of the real conditions of the American tropics. But the greatest hope that some of us long-time residents of the Caribbean area see in the foundation of the new institute lies not so much in what it can do in the way of publishing pretty bulletins and reports or even in the development of improved plant varieties; but rather in the courage which it might display in facing some of the broader problems of tropical agriculture.

Consider, for example, the problem of fuel and power: Every one who has carefully studied the agricultural economics of countries like Costa Rica inevitably concludes that the scarcity and high price of fuels constitute the greatest single barrier against a real development of the country. Regardless as to whether we talk of railroad or automotive freight-rates or about the development of small local industries, we always come back to the painful fact that the only fuel locally produced is wood-fuel and that its continued use at anywhere near the present rate will soon create a terrific problem in forest depletion, soil erosion, irregular stream flow, etc. And thus we also inevitably reach the conclusion that any honestly conducted program of agricultural conservation and development in Costa Rica must first contemplate making available large blocks of cheap hydroelectric power, in order that the people shall not only have an inexpensive source of heat to cook with, but also that electrification be applied to such things as the small farm, the small rural industry and to all railroads in the country not already electrified. Furthermore, any honest-to-goodness program of tropical sanitation will have to dispose of large amounts of cheap chlorine for water disinfection and as the means of oxidizing the harmful residues of the coffee industry. This is intimately tied up with the cost of electricity, as chlorine is a product of the electrolytic cell.

The lands acquired by the Institute of Tropical Agriculture at Turrialba are located in the middle of a region rich in waterfalls which have never been developed because of the financial limitations of the province where Turrialba is located. But we already have an example in the nearby province of Alajuela, where there is a publicly operated electric plant that was built some fifteen years ago by a German-Swiss company. This plant has not only paid for itself, but has also demonstrated that electricity may be produced in the tropics at a fraction of the cost of even the so-called "yardstick" plants in the United States like T.V.A. Likewise, the government-owned and -operated Ferrocarril Electrico al Pacifico offers another definite example of how a relatively modest investment in electrical transportation facilities pays back a thousand-fold in the tremendous agricultural development made possible on the Pacific sector of Costa Rica.

Considering the ease with which the new Institute of Tropical Agriculture could obtain the loan of technical talent from other Federal agencies, such as the T.V.A. or the Rural Electrification Administration and the low interest rates at which projects sponsored by the Federal Government may be financed, it seems to the writer that the new institute has in its hands a brilliant opportunity to do some real progress in tropical agriculture if it could only muster the courage to abandon the beaten track and face the economic realities of the Turrialba valley. Naturally, the suggested course is not the easiest; as in order to put across the project, the institute might have to fight both private electric utilities and certain foreign financed agricultural ventures in which the workmen are kept under the patriarchal conditions which prevailed in Europe during the Dark Ages. But then, again, what ever really great institution has ever grown up by following the beaten track?

Furthermore, any endeavor that the agricultural

institute can sponsor tending towards the more intelligent utilization of the natural resources of the Turrialba Valley will be supported by the common people of Costa Rica, who are as a whole industrious and literate.

RAFAEL W. KEITH

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SCIENTIFIC BOOKS

METEOROLOGY

Harvard Meteorological Studies, No. 6. Heat Transfer by Infrared Radiation in the Atmosphere. By WALTER M. ELSASSER, Harvard University, Blue Hill Meteorological Observatory, Milton, Mass. 106 pp. Appendix, bibliography and copy of the Atmospheric Radiation Chart, second edition. 1942.

THIS publication comprises three parts, more or less independent, namely: Part I, "Principles of Radiative Transfer." Part II, "Structure and Absorption of Infrared Bands." Part III, "The Measurement of Atmospheric Emission." Then follows the Appendix, descriptive of a mechanical computing device, the Bibliography and the Radiation Chart.

Students of atmospheric radiation have been under deep obligation to Dr. Elsasser for several years on account of his excellent chart. Making use of additional observations and reconsiderations the chart has now been recomputed, and has undoubtedly become a far more trustworthy representation of the complex problem of atmospheric radiation. The author calls attention, however, to the need of much additional observation to cover many doubtful matters in this difficult field.

Part I begins with a demonstration of Kirchhoff's principle: "The ratio of emission and fractional absorption in any direction of a slab of any thickness in thermodynamic equilibrium equals the black body intensity." To this reviewer the author's demonstration lacks something of completeness. He considers a constant temperature enclosure with totally opaque walls, and shows that the second law of thermodynamics requires that the transfer of energy from one wall to the opposite must equal the return. He says: "Therefore, the emission of a perfectly opaque wall is generally independent of the optical properties of the wall." Suppose one wall was of soot and the opposite wall was of polished silver. The emission of the soot would be many times as intense as the emission of the silver. But within the enclosure by reflection, supplementing emission, the flux from the silver is built up to be the flux from an absolutely black body, and the same holds to a minor extent with the soot. Had this point been established and made clear, the author could have passed logically to his statement in the next paragraph: "We call I_b the intensity of this beam which, in thermodynamic equilibrium, is just the black body intensity." From this statement the Kirchhoff expression $E/A = I_b$ follows at once, as the author shows.

The author continues in Part I with Schwarzschild's equation of radiative transfer and its integration under the particular conditions which prevail in the atmosphere. Planck's law connecting temperature and frequency, Stefan's fourth power law for total radiation are considered, and a valuable table follows representing for the black body the integration of radiation intensity by wave number and the change of intensity with temperature.

Transfer of monochromatic radiation, and of non-monochromatic radiation are treated mathematically, bringing in, of course, the important exponential principle of Bouguer and Lambert and the relation: "For isotropic radiation the flux [total over a hemisphere] is π times the intensity of a straight beam," and developing an important set of functions useful in radiation transfer problems including the so-called recursion formula. These mathematical steps lead on to the general transfer problem of the flux emitted by an atmosphere of arbitrary constitution, and the first mention of the Atmospheric Radiation Chart.

And now observation comes to the fore with its showing of the special radiative and absorptive properties of water vapor, carbon dioxide, ozone and the permanent gases oxygen and nitrogen. The application of these properties in the Radiation Chart and the use of the chart for particular problems is discussed. The reviewer, however, believes that ordinary users of the chart would have appreciated several completely worked-out numerical applications of it to specific examples from actual observations. Something of this, indeed, is given in Table 2, but more extensive numerical examples would be helpful.

Two aspects of absorption in spectral lines, namely, broadening under pressure and Doppler effects of motion, are found to be of some importance concerning atmospheric radiation problems, and are treated by the author at considerable length. Then comes the discussion of overlapping spectrum lines in band spectra, ending in the derivation of a formula for band absorption in the most general case. Following is a reference to the effects of pressure and temperature on absorption coefficients of the atmospheric constituents.

Water vapor, as is well known, presents a very difficult problem in atmospheric radiation and absorption. Rapidly falling off with altitude, presenting many bands of line structure, subject to rapid and large changes of quantity from day to day, affected by both pressure and temperature in its absorption, effective in both solar and terrestrial spectral ranges,

one might well fear that it would prove totally unmanageable in a chart. But several researches of great merit, referred to by the author in warm terms of appreciation, have furnished so considerable an experimental background that empirical formulae fairly representative of water vapor radiation and absorption have been derived. This part of the author's publication occupies 12 pages with several diagrams and tables.

Carbon dioxide and ozone compared to water vapor are simple problems and are briefly treated.

In Part III, instruments and methods for measuring radiation and absorption of atmospheric constituents are discussed at considerable length and with good discretion.

A brief description is given of the important instruments used for carrying out the extensive numerical calculations for the chart. Then follows a bibliography of 123 entries, and finally the highly valuable chart.

All those interested in atmospheric radiation researches will find Dr. Elsasser's publication timely and invaluable. No doubt as research goes on he will from time to time revise and improve it.

C. G. ABBOT

SMITHSONIAN INSTITUTION

ANOXIA

Anoxia: Its Effect on the Body. By EDWARD J. VAN LIERE. xiii + 269 pp. Chicago: University of Chicago Press. 1942.

THE subject of oxygen deprivation, and its physiological and psychological effects, has assumed great importance in recent years in medicine and related fields. With the present war, this subject has become of increasing importance because of its role in high altitude flying in aviation. This timely review is more comprehensive than any other single work on this subject in the English language. It comprises a systematic presentation of the results of experimental studies in this field, arranged according to the physiological systems of the body.

Following a brief historical introduction, there is a short chapter dealing with the definitions of terms. The familiar classification of the various types of anoxia, according to Barcroft and to Peters and Van Slyke, is then given with a brief review of the physiology of respiration. The subject of cellular oxidation is but barely mentioned. Schmidt's classification of anoxia into the fulminating, acute and chronic varieties is then described. Here (p. 16) it should be pointed out that the term "chronic anoxia" should not be used in a sense almost equivalent to "chronic mountain sickness" as necessarily resulting in adverse symptoms. Permanent residents of high altitudes live in a condition of chronic or constant anoxia and most of them, at least up to a certain altitude, do not show signs or symptoms related to oxygen want. There follows a chapter on the experimental methods of producing anoxia. In appraising the various methods (p. 27) the author fails to bring out the fact that with the rebreather, the degree of anoxia progressively increases at such a rapid rate, and the duration of an experiment is therefore so short as to enable the subject to compensate for the effects by exerting extra effort for a short time until the final stages of deterioration. During the first World War, the rebreather tests resulted in a false impression as to the altitudes at which aviators could remain for any length of time and still maintain their physical and mental capacities. Experiments of longer duration reveal a much lower "ceiling" for the average pilot or unacclimatized subject.

The remaining chapters deal with the effects of anoxia on the various physiological systems, beginning with the morphology and chemistry of the blood, the circulatory system, with separate chapters on blood pressure and on the lymph. In the chapter on the effects of anoxia on the blood, the author points out the necessity for distinguishing clearly between "oxygen content" and "oxygen capacity" of the blood. He erroneously states, however (p. 52), that "In Keys group the oxygen *content* of the blood increased an average of 25 per cent." It was actually the oxygen *capacity* which increased.

In several cases in which more than one factor is involved the author does not bring out the exact role played by each variable. Thus in his discussion of the changes in alveolar pCO_2, pO_2 and arterial O_2 content at high altitudes (p. 102), the author does not clearly distinguish between the effect of (a) increasing ventilation on lowering the alveolar pCO_2 and concurrently increasing the alveolar pO_2 and of (b) the Bohr effect, which leads to a greater O_2 content of the blood when the arterial pCO_2 is decreased but the arterial pO_2 is held constant. He tries to cover the whole situation merely with the Bohr effect, which *per se* has nothing to do with alveolar pressures.

The following chapters deal with mountain sickness, altitude sickness and acclimatization. In comparing the effects of acclimatization to oxygen want with acclimatization to carbon dioxide, the author states (p. 155) that certain similarities are to be expected since "in both cases there is some increase in tissue acidity." It is very questionable whether anoxia, unless it be of very serious degree, gives rise to tissue acidity; on the contrary, the increase in pulmonary ventilation results in "blowing off" of CO_2 and thus a decrease in acidity of the body fluids.

Probably the most complete and authoritative

of the book is the chapter dealing with the effects of anoxia on the digestive tract. The author himself and his co-workers have studied this subject quite intensively over a period of years.

The remaining chapters deal with the secretion of urine, the endocrine glands, metabolism, heat regulation, nutrition, water distribution in the body and the nervous system.

In the last chapter the author discusses the physiological effects of anoxia on the nervous tissue, its circulation and function. This is followed, without adequate integration, by a discussion of the psychological effects of anoxia. This section, as the author admits, is especially lacking in detail and completeness of bibliography. The most interesting observations in this field relate to the striking and insidious changes in behavior produced by a lack of oxygen to the nervous tissue. The author fails to interpret the intimate relationship which must exist between an adequate and constant supply of oxygen to the nervous tissue and mental functions, such as memory, judgment and reasoning, which are essentially psychological in nature. This is important, not only from the point of view of research relating to the possible cause of certain mental disorders, but also in an understanding of the very foundation of mental processes or behavior in general.

The discussion of the visual effects is particularly sketchy and incomplete. Little attempt is made to interpret the role of these findings in an understanding of the nature of the visual mechanism itself. Several errors in this last chapter might be pointed out. Psychological tests, such as speed of apprehension, memory tests and repetition of auditory patterns, commonly termed mental tests, are erroneously referred to as "psycho-somatic tests" (p. 234). In reporting the various studies on sensory function, the expression "decrease in threshold" is used where "increase" is obviously meant (pp. 244, 246).

The author might have emphasized more clearly that the great variations found in the responses and adaptive processes to anoxia are not only related to the type of stimulus (acute, intermittent, chronic) but are also attributable in great part to the different *degrees* of anoxia used in experiments and found at different levels of altitude.

The chief criticism concerning the style in which this monograph was written is that frequently mere statements of experimental results are given, without mention of the methods employed by the investigators, or critical appraisal of other factors which might be of value in judging their validity. This deficiency is noted particularly when opposing experimental results are cited. More interpretation and integration of the findings of the various investigators would have improved this monograph.

For the reader who is not familiar with the literature, this monograph provides a valuable summary. It is, however, not as complete in its bibliography as might be desired. A number of important studies are omitted which would have assisted the author in interpreting several controversial topics. In the section on hematology, for example, the important paper by Talbott (1936) from the International High Altitude Expedition is not mentioned. Also, a great part of the work done in Peru has been neglected. It seems regrettable that Hoff and Fulton's more complete and excellent "Bibliography of Aviation Medicine," also published in 1942, was not available to Dr. Van Liere during his preparation of this book. Also the lack of an authors' index in a work of this kind is unfortunate.

The need for a current book reviewing the work done on this subject has been very great. In spite of the above criticisms, Dr. Van Liere's monograph serves as a useful introduction to the literature on the physiology of oxygen deficiency. It will be of value not only to research workers in physiology and psychology, but also to those concerned with the problems of high altitude in aviation. The clinician, as well, will find it of interest in view of the role of anoxia in various diseases.

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SPECIAL ARTICLES

ABOLISHMENT OF ALIMENTARY LIPEMIA FOLLOWING INJECTION OF HEPARIN

DURING the course of determining red cell circulating mass in dogs by the donor-isotope red cell procedure, an occasional animal of irregular eating habits showed a marked lipemia in the initial control blood sample. When injected with whole blood containing tagged cells, in the instances in which heparin was used as an anticoagulant, this lipemia had disappeared completely in the blood sample taken three to five minutes later. This phenomenon was so striking, even in the instances where the degree of lipemia was such that the plasma was suggestive of light cream, that it seemed advisable to determine how specific the reaction might be, ruling out any special donor-recipient peculiarities which might be responsible for the change.

In the first experiments the lipemia was due to the dilatory eating habits of the animals. Later, in order

to be able to depend on the presence of lipemia the food was not given in the afternoon as usual but reserved until morning, or sometimes an extra meal consisting of 100 gms each of salmon bread, Klim and cod liver oil was fed from two to four hours before sampling.

Both blood samples were taken from the same jugular vein, the needle being left in place. The injected material was also introduced through this needle. The degree of lipemia was not quantitated except roughly as follows: 0 – water clear plasma; 1+ – slight turbidity; 2+ definite lipemia; 3+ sufficient lipemia to obscure the meniscus of the plasma completely; 4+ – same as 3+ but with frank lipid layer on top the plasma layer.

In Table 1 is shown a series of reactions in which heparinized whole blood from a number of donor dogs was injected, as well as plasma from heparinized blood. Two makes of heparin were used, those experiments marked with * were carried out with a preparation obtained from the Hynson and Westcott Company of Baltimore, while those marked ** were done with material of considerably greater specific potency (1 mgm = 110 units) obtained from the Connaught Laboratories of Toronto. In all experiments in which heparinized whole blood or plasma was administered the lipemia was abolished. The reaction did not occur on the injection of washed red cells derived from heparinized blood nor after injection of any fractions derived from citrated blood.

It was finally found that the same amount of heparin (**2.4 mgm) as used in the earlier experiments when dissolved in saline and given by vein would in itself abolish the lipemia.

Addition of 5 mgm of heparin to 5 ml of lipemi plasma *in vitro* with mixing showed no reaction o standing. Neither did mixing of heparin with nor lipemic plasma and subsequent mixing with lipemi plasma result in the clearing of the latter.

The time interval of 5 minutes elapsing betwee the injection of the heparinized material and samplin is more than necessary for the reaction to occur. I three of the experiments shown in Table 1, sampl were taken at ½, 1, 1.5, 2 and 3 minutes as well as minutes. In each of these instances the lipemia w

TABLE 1

LIPEMIA BEFORE AND AFTER ADMINISTRATION OF HEPARIN

Date	Dog		Material introduced	Degree of lipemia		Donor source
				Initial	Final	
1/8	40–115	*	20 ml heparinized whole blood	3+	0	39–266
1/20	41–164	**	" " " "	3+	0	1–J
1/19	1–J	**	" " " "	3+	0	39–266
1/4	1–K	*	" " " "	3+	0	39–266
1/8	1–K	*	" " " "	4+	0	39–266
1/19	40–115	**	20 ml heparinized plasma	2+	0	1–J
1/19	41–164	**	" " "	4+	0–1+	39–266
1/22	39–266	**	" " "	4+	1+	38–137
1/20	39–196	**	" " "	4+	0–1+	40–149
1/13	1–J		20 ml citrated whole blood	2+	2+	pooled
1/13	38–137		" " " "	3+	3+	"
1/18	39–196		" " " "	3+	3+	39–266
1/13	40–115		25 ml citrated plasma	4+	4+	pooled
1/14	39–57		" " "	4+	4+	"
1/22	39–266	*	20 ml washed heparinized cells	4+	4+	38–137
1/19	40–149	*	" " " "	3+	2+	39–266
1/13	1–K		25 ml citrated washed cells	2+	2+	pooled
1/18	1–K		" " " "	4+	4+	"
1/23	40–115	**	20 ml citrated plasma (itself lipemic) to which was added 250 units of heparin	2+	0	39–266
1/23	41–164	**	250 units of heparin in 5 ml saline	2+	0	....
2/11	39–57	**	" " " " " " " "	4+	0	....
1/28	40–115	**	" " " " " " " "	1+	0	....
2/1	38–137	**	" " " " " " " "	3+	0–1+	....
2/11	39–266	**	" " " " " " " "	3–4+	0	....

practically absent in the recipient's blood at the of 1 minute.

On a basis of the data presented here it would be wise to speculate as to the nature of the mecha involved in the abolishment of lipemia from the b of dogs as a result of the injection of mode amounts of heparin. Fractional lipid analyses plasma taken before and after injection of hep are being carried out and will be reported at a date.

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PRODUCTION AND TREATMENT OF GRA LOCYTOPENIA AND ANEMIA IN RAT FED SULFONAMIDES IN PURIFIED DIETS

THE production of granulocytopenia and an in rats through the use of sulfanilylguanidine or cinyl sulfathiazole in purified diets and treatme the animals with whole dried liver or liver frac have been reported from this laboratory.[1] The

[1] S. S. Spicer, F. S. Daft, W. H. Sebrell and L Ashburn, *Pub. Health Rep.*, 57: 1559, 1942.

t study concerns the production and treatment of milar blood dyscrasias in rats receiving sulfathiaole, sulfadiazine or sulfanilamide.

PRODUCTION

Albino rats were weaned at 21 to 27 days and placed one of two experimental diets. One hundred and e rats were given a diet composed of glucose ("Cereose") 72 per cent., "vitamin-free" Smaco casein 18 r cent., cod liver oil 2 per cent., cottonseed (Wesson) oil 3 per cent., salt mixture No. 550[1] 4 per cent. and either sulfathiazole, sulfadiazine or sulfanilamide at a level of 1 per cent. Twenty-six additional rats received one of these three sulfonamide drugs in a similar diet, except that it contained 25 per cent. Smaco casein and 65 per cent. glucose ("Cerelose"). Each rat received a daily supplement of 100 micrograms of thiamine hydrochloride, 200 micrograms of riboflavin, 100 micrograms of pyridoxine hydrochloride, 200 micrograms of calcium pantothenate, 1 mg of niacin and 10 mg of choline chloride. The rats were weighed three times weekly and after weight gain had ceased or when the animals appeared sick, blood counts were made and in some of the animals were repeated at irregular intervals. The techniques were the same as those previously employed.[1] The term "blood dyscrasia" is used in this report to denote a "severe granulocytopenia" or a "severe anemia" or both combined. "Severe granulocytopenia" is used to indicate a total polymorphonuclear granulocyte count of less than 150 cells per cu mm. The term "severe anemia" is used to indicate hemoglobin or hematocrit values of less than 7.5 grams per 100 cc or 25 cc per 100 cc, respectively. Milder degrees of granulocytopenia and anemia are not considered in this report.

From the data obtained, no marked differences have been noted between the incidence of blood dyscrasias in the rats given 18 per cent. Smaco casein and those given 25 per cent. Smaco casein in these diets; also the response to treatment has been similar. The data concerning both groups therefore have been considered together. Of the 127 animals on experiment, 22 were excluded from consideration because they died without blood counts having been made. Fifty-two animals developed a blood dyscrasia. The remaining animals not showing such severe blood changes have been under observation from 39 to 169 days. Twenty-nine of these animals have died and the remaining 32 have been sacrificed. Blood dyscrasias have been found as early as the tenth experimental day and as late as the eighty-sixth. The data in Table I do not include less severe degrees of granulocytopenia and anemia which were observed frequently.

It is to be noted that under these experimental conditions, the incidence of blood dyscrasias was very much less in the sulfanilamide group than in either the sulfathiazole or sulfadiazine groups. For these groups of animals the incidence of severe granulocytopenia was greater in those receiving sulfathiazole than in those receiving sulfadiazine, while the reverse

TABLE I

THE OCCURRENCE OF SEVERE GRANULOCYTOPENIA AND ANEMIA IN RATS INGESTING A SULFONAMIDE-CONTAINING, PURIFIED DIET

Sulfonamide contained in the diet	Total number of rats studied	Number of rats with a blood dyscrasia (severe granulocytopenia or severe anemia or both)	Number of rats with a total polymorphonuclear granulocyte count of 0–150 cells/cu mm	Number of rats with a hemoglobin or hematocrit of less than 7.5 gms/100 cc or 25 cc/100 cc respectively
Sulfathiazole	34	28	22	10
Sulfadiazine	36	22	9	16
Sulfanilamide	35	2	2	1

was true of the incidence of severe anemia. Blood smears from severely anemic animals usually revealed marked abnormalities in the size, shape and staining reactions of the red blood cells and an increased number of nucleated forms. In the animals with severe granulocytopenia, a leucopenia with a mean value of about 4,000 cells per cu mm was usually present.

TREATMENT

Of the 52 animals that developed a blood dyscrasia, 30 were not treated and died. Of the remaining 22 animals, 17 were treated for a severe granulocytopenia, four for a severe anemia and one for both conditions combined. Fifteen of the 18 animals with granulocytopenia[2] were treated by the daily oral administration of 1 cc of an aqueous suspension containing 0.25 gm per cc of Wilson's solubilized liver,[3] or smaller amounts of more refined liver fractions; three were given a daily oral supplement of 1 gm of whole dried brewers' yeast. The treatment period was four days, during which time the animals continued to eat the sulfonamide-containing diet. Counts were made immediately before treatment and again at the end of four days of treatment. Five of the liver-treated animals failed to survive for more than three days. Recounts of the 13 animals which did survive the treatment period revealed a significant response in every instance. Table II contains the values for the liver-treated group before and after treatment. Five rats were severely anemic and four of them[4] were

[2] The animal with granulocytopenia and anemia combined is included here.

[3] Furnished through the courtesy of Dr. David Klein, Wilson Laboratories.

[4] The animal with granulocytopenia and anemia combined is included here.

treated with 1 cc of Wilson's solubilized liver suspension for each of 10 successive days. The data for these animals are given in Table II, and as noted all showed some response at the end of five days; near normal levels were observed after 10 days of treatment. The one anemic animal not included in the table had an initial hemoglobin value of 6.5 gms per 100 cc. After 10 days of treatment with another fraction, it had fallen to a level of 2.3 grams per 100 cc, the hematocrit remaining unaltered at 18 cc per 100 cc. Treatment with the suspension of Wilson's solubilized liver was then given for 12 days. The hemoglobin level rose to 11.5 grams per 100 cc and the hematocrit to 56 cc per 100 cc. All the 14 animals treated with liver fractions and the three treated with brewers' yeast showed a resumption of growth and a marked clinical improvement.

The mechanism of action of the sulfonamide drugs in producing these blood dyscrasias as well as other toxic manifestations is obscure. It has been suggested[1] that there may be an indirect toxicity such as an interference with tissue enzyme systems. Also the possibility was considered that such an indirect toxicity, a direct toxicity and a reduction of intestinal synthesis of essential factors might all be involved.

SUMMARY

(1) A severe granulocytopenia or anemia, or both, have been produced in rats fed sulfathiazole, sulfadiazine or sulfanilamide at a 1 per cent. level in purified diets.

(2) Treatment with certain liver fractions orally has succeeded in correcting the granulocytopenia in four days and the anemia in about 10 days in spite of continued ingestion by these animals of the sulfonamide-containing diet.

TABLE II

BLOOD VALUES SHOWING A RESPONSE OF SEVERELY GRANULOCYTOPENIC OR ANEMIC RATS TO TREATMENT WITH LIVER FRACTION

	Number of experimental rats	Before treatment		Number of days of treatment	After treatment		Normal values cited in literature	
		mean	range		mean	range	mean	range
Total polymorphonuclear granulocytes per cu mm	10	29	0–103	4	3,203	1,550–5,760	3,465	1,200–6,82
Percentage of polymorphonuclear granulocytes	10	0.8	0–3	4	30.1	21–41	29.9*	15–45
Total white blood cells per cu mm	10	3,808	550–7,320	4	10,550	4,950–16,950	11,590*	8,000–15,0
Hemoglobin in gms per 100 cc	4‡	7.0	6.4–7.4	5	9.1	8.2–9.7	15.6*	14.0–17
				10	13.6	13.0–14.1		
Red blood cell volume (Hematocrit) in cc per 100 cc	4‡	24	21–27	5	35	33–39	50†	.....
				10	45	41–47		

* R. A. Scarborough, *Yale Jour. Biol. and Med.*, 3: 267, 1931.
† A. J. Creskoff, T. Fitz-Hugh, Jr., and E. J. Farris, in "The Rat in Laboratory Investigation," edited by J. G. Gr and E. J. Farris, p. 351. Philadelphia: J. B. Lippincott Co., 1942.
‡ One additional animal is discussed in the text.

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SCIENTIFIC APPARATUS AND LABORATORY METHOD

THE DETERMINATION OF CELL VOLUME AND HEMOGLOBIN ON THE SAME DROP OF BLOOD

AN adequate clinical description of the erythrocytes in blood requires the determination of the cell volume, the hemoglobin content and the cell count.

A method for determination of cell volume, called the cell opacity method, has been described recently.[1] It involves the measurement in a photoelectric colorimeter of the transmission of light at a wave length of about 660 mμ through a suspension of blood in ci solution. The cell opacity thus recorded is pr tional to the cell volume, which can be read fr line or table showing this relationship. This me requires only 5–25 cmm of blood.

A method for the determination of hemoglobi the measurement in a photoelectric colorimeter o transmission of light of about 540 mμ has bee scribed by Evelyn.[2] This determination requi similar amount of blood.[3]

[1] A. T. Shohl, *Jour. Lab. and Clin. Med.*, 25: 1325, 1940.

[2] K. A. Evelyn, *Jour. Biol. Chem.*, 115: 63, 1936; K. A. Evelyn and H. T. Malloy, *ibid.*, 126: 655, 193

It is convenient to make both measurements on the same sample of blood. After the cell opacity has been determined, the blood is hemolyzed and the hemoglobin quantitated with only a slight modification of Evelyn's technique. The blood is hemolyzed by the addition of one drop of saponin solution to the red cell suspension. Merck's saponin has been found to be satisfactory, but different samples have different degrees of activity and deteriorate at different rates. We have used an aqueous solution containing approximately 1 gm of saponin made up to 10 cc. It will retain its efficacy for 3–4 weeks or longer if kept in the refrigerator. The criterion of its adequacy is that one drop should hemolyze the blood and give a clear solution in less than one minute. The oxyhemoglobin thus obtained is made alkaline by the addition of one drop of concentrated ammonia (20 per cent. ammonia gas). Saponin and ammonia are added to the blank containing citrate solution only, and the colorimeter adjusted with a filter transmitting light at or near 540 mμ. The tube containing the sample is then inserted in the colorimeter and the galvanometer reading is interpreted in terms of hemoglobin concentration.[3] Parallel determinations by Evelyn's method and by this variation of it give the same values for hemoglobin.

This method uses such a small amount of material and is so easy to carry out that it may be surmised that accuracy has been sacrificed to convenience. Such is not the case. Each of the methods has been checked, the cell opacity against the hematocrit method and the hemoglobin against the oxygen capacity method. The results obtained show no greater errors than the standards against which they were compared. To obtain maximum accuracy it is necessary to have carefully calibrated pipettes. However, if the readings of duplicate or multiple determinations made with different pipettes on aliquots of the same blood *in vitro* are not in agreement for the cell opacity, it will be found that the values for the amount of hemoglobin show corresponding variations. Each pair of determinations shows the same relationship because they are made on the same sample, regardless of its size. Thus, even if the pipettes are inaccurate, the relationship between the cell volume and hemoglobin concentration is accurately shown, and for some purposes it is more important to know the relative than the absolute values. Furthermore, two samples of capillary blood taken from the same individual may contain different amounts of plasma, tissue fluids and muscles. This source of error in sampling also is eliminated by the combined method.

[3] The full technique for use of the photoelectric colorimeter is described in the manual which is supplied with both the Evelyn and the Klett-Summerson colorimeters.

Attempts have been made to use the transmission of light through a cell suspension for erythrocyte count with the Evelyn colorimeter[4] or the Exton scopometer.[5] These methods have been unsuccessful[6] in so far as they give approximately correct values only when the cells are of constant and normal size. It is primarily the total mass of cells and only secondarily their size which determines the amount of light transmitted.[7] This is shown clearly in the example of rat blood, which gives values similar to those of human blood for cell opacity and has a similar cell volume, but a cell count nearly twice as high.[8] However, if dealing with physiological conditions in which the size of the cells does not vary, the cell opacity gives a correlation with cell count as well as with cell volume.

Most physicians are more familiar with the clinical interpretation of cell count than of cell volume. The erythrocyte count as obtained by the cell opacity method may be used as an approximation for orientation, assuming the cells to be of normal size. For an accurate description of the erythrocytes in blood a cell count should be done in conjunction with the determination of cell volume and hemoglobin content. For the latter two the combined method given above has proved to be simple, accurate and economical of both time and material.[9]

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GROWTH OF ORCHID SEEDS AFTER DEHYDRATION FROM THE FROZEN STATE

CERTAIN orchid seeds have germinated after eight and even fourteen years[1] when stored under optimum conditions, that is, in dry air at 46° F. In general, however, their lifespan is considerably shorter. A method which has proven outstandingly successful in the preservation of such biological materials as bacteria, filterable viruses and serum proteins is that of desiccation from the frozen state. It was thought to be of both practical and theoretical interest to determine, firstly, whether orchid seeds could survive and

[4] R. V. Christie and K. A. Evelyn, *Jour. Clin. Invest.*, 13: 704, 1934.
[5] W. G. Exton, *Am. Jour. Clin. Path.*, 7: 42, 1937.
[6] Unpublished data of Dr. G. M. Guest.
[7] D. Drabkin and R. B. Singer, *Jour. Biol. Chem.*, 129: 739, 1939.
[8] A. T. Shohl, K. D. Blackfan and L. K. Diamond, *Proc. Soc. Exp. Biol. and Med.*, 45: 383, 1940.
[9] Recent notes by F. T. Hunter, *Jour. Clin. Invest.*, 19: 691, 1940, and D. H. Duffie, *Jour. Am. Med. Asn.*, 119: 493, 1942, have shown the possibility of substituting sodium carbonate for ammonia. This procedure recommends itself in a laboratory where nitrogen determinations are being made. At present we add 1 drop of 5 per cent. sodium carbonate instead of ammonia.
[1] L. Knudson, *Amer. Orchid Soc. Bul.*, 9: 36–38, 1940.

grow after subjection to this process; and secondly, if so, whether the lifespan was increased. This paper reports the survival and growth of orchid seeds when vacuum dried after freezing at low temperatures.

A miniature, "lyophile" apparatus[2] described by Flosdorf and Mudd[3] was used, and with slight modifications the procedures outlined by these authors were applied to orchid seeds. The seeds used were the results of two primary crosses, *Cattleya Loddigesii* crossed upon *Cattleya Schroederiana* and *Laelia anceps* crossed upon *Cattleya Trianaei*. These species are natives of either Brazil, Colombia or Mexico and grow in climates where freezing temperatures are unknown or unusual.

In comparison with seeds of most flowering plants, those of orchids are very small. The ones used in this experiment measured on an average 527.7 μ by 73.5 μ. They had been stored for 7 months in a glass jar in a refrigerator. Their moisture content was not determined before they were "lyophiled" nor after, at the time of planting. The percentage of moisture in seeds of another cross stored in the refrigerator under similar conditions for 4 months has been found to be 1.5 per cent. The residual moisture which Flosdorf and Mudd found remaining in bacterial preparations after subjection to the "lyophile" process was 0.5 per cent.[3]

Two sets of tubes were prepared for desiccation. In one, sterile blood serum (commonly utilized in the preservation of bacteria by this method) was used as the suspending fluid and in the other, autoclaved coconut liquid. A mass of seeds which would approximate two drops of water in volume was placed in each tube. The tubes were plugged with sterile cotton and thoroughly agitated so that the seeds became completely coated with the liquids. This required some manipulation, for, due to the structure of orchid seeds, the outer alar cells resist wetting. The tubes were plunged into the dry ice bath of the main condenser of the apparatus at a temperature of −78° C. for about three minutes until thoroughly frozen. They were then attached to the vacuum manifold, being kept immersed at the same time in a dry ice bath maintained at −5° to −10° C. The vacuum pump was started and dehydration allowed to proceed for three to four hours. At the end of the second hour, the tubes were removed from the cold bath and held at room temperature. They were sealed off while under vacuum with an oxygen flame. These procedures (using the same volume of suspending fluid, degree of vacuum and time of desiccation) had given consistently satisfactory results with this apparatus in the preservation of bacteria.

[2] Made available through the courtesy of the Department of Bacteriology at the University of Washington, Seattle, Wash.

[3] E. W. Flosdorf and S. Mudd, *Jour. Immunol.*, 29: 389–425, 1935.

Half of the tubes were stored for future tests of viability. The other half were broken open and the contents planted. Following Knudson's technique for growing orchid seeds non-symbiotically, they were planted aseptically on a nutrient agar medium using a slightly modified Knudson's Solution B[4] with sucrose as the sugar and the pH 6.3. Some of the contents of the tubes were planted directly from the tubes while others were first sterilized with hypochlorite solution after Wilson's method.[5] No immediate contamination resulted from either of these procedures.

None of the seeds which had been immersed in blood serum germinated. All but one tube of those suspended in coconut liquid not only germinated but with the exception of a few flasks which later became contaminated, grew satisfactorily. One flask containing seeds of *Cattleya Loddigesii* crossed upon *Cattleya Schroederiana* germinated within the short period of seven days, which was four days before the control showed signs of germination. After four months most of the seedlings had two leaves and one or two roots and were sufficiently large to remove from the containers and plant into community pots.

Although various kinds of seeds have survived exposure to low temperatures after varying degrees of drying,[6] to our knowledge this is the first time that seeds have been subjected to the "lyophile" process and have grown. Seeds of the tuberous begonia and snapdragon, which are roughly 2 to 10 times the size of orchid seeds, were similarly treated but failed to survive and grow.

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[4] L. Knudson, *Bot. Gaz.*, 73: 1–25, 1922.

[5] J. K. Wilson, *Amer. Jour. Bot.*, 2: 420–427, 1915.

[6] B. J. Luyet and P. M. Gehenio, *Biodynamica*, Normandy, Missouri, 1940.

[7] Formerly with the University of Washington.

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